that the mere adhesion between the smooth wheels and smooth rails was completely insufficient to prevent slipping.

In the year 1812 William Hedley was viewer at the Wylam Colliery, and in order to reduce the working expenses he endeavoured to construct an engine to haul the coal waggons from the colliery to the river, and to do it cheaper than by horse haulage. At this time he had a knowledge of what others had done in this direction, but was forcibly impressed with the idea that the weight of an engine was sufficient for the purpose of enabling it to draw a train of loaded waggons. After having made successful experiments to prove the idea correct, he set to work and constructed his first engine, which, when completed, did not prove a success owing to shortness of steam, and a second one was made. The second one, the well-known "Puffing Billy," was put to work in May, 1813, and was a complete success. This may be safely called the first practical and efficient locomotive ever constructed. It had a return-tube boiler of wrought iron, vertical cylinders, and was placed on four wheels. Very soon after the engine commenced to work the exhaust steam was turned into the chimney to create a blast on the fire. This engine worked nearly continuously until 1862, when it was bought, and has now found an honourable resting-place in South Kensington Museum.

Puffing Billy was put to work in 1813, nearly a year before Stephenson's first engine was tried at Killingworth in 1814, thus proving without doubt that William Hedley was the first man to construct the first practically successful locomotive engine, and the first economical substitute for animal power.

It should not be thought that our author claims for Hedley the fame of being the first to develop the railways. Puffing Billy was at work sixteen years before the celebrated Rainhill contest took place, and ten years before locomotives were allowed to work the goods traffic on the Stockton and Darlington Railway.

Stephenson's success may be dated from the Rainhill contest in 1829; and he was one of the first men to bring the present railway system forward and develop it. At the same time William James must not be forgotten; he surveyed the Manchester and Liverpool Railway before Stephenson was placed in charge of the Railway Works, and had it not been for a difference of opinion on certain technical points, William James would have been the engineer of the line until open for traffic. Again, William James went to see Stephenson's engine, before Stephenson came to Liverpool, finding him an intelligent working man and the engine a success, he brought Stephenson to Liverpool, where he eventually commenced his successful career.

The author is to be congratulated on having proved his case, and in the preface he truly says: "Without William Hedley, George Stephenson might have lived in vain. It was William Hedley who gave the locomotive its life and power, and made the work of other men possible."

The book is very interesting, and is useful as a book of reference, the appendix containing extracts from the opinions of many writers, and letters from men able to give information on the subject. This little book will prove useful to all who wish to know the facts concerning William Hedley and his inventions. N. J. L.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Shotfiring in Mines

FOR some time past I have been conducting a series of shot-firing experiments at Dowlais and elsewhere on behalf of the Royal Commission on Accidents in Mines. Towards the end of August last Prof. C. G. Kreischer, of Freiberg in Saxony, visited me at Cardiff for the purpose of conferring with me on the coal-dust question. The experiments at Dowlais have a direct bearing on that subject, so, after pointing out to Prof. Kreischer the perfectly private nature of the investigation and the delicate position in which I would be placed were the results allowed to transpire through any channel other than the Royal Commission, and having received his assurance that such a contingency was impossible as far as he was concerned, I asked him to accompany me to Dowlais, so that he might witness some of the experiments on August 28 and September 1.

On the second (?) day Prof. Kreischer asked my permission to write to his friends in Germany, suggesting that they might make a few similar experiments privately in an apparatus that had been set up at Zwickau, at the expense of the Saxon Government, for the purpose of conducting a series of experiments with coal-dust. He again assured me that no publication of results would take place until after those obtained here were made known, and offered, if I had the least doubt as to the integrity of his friends, not to put it in their power to anticipate our results by not writing to them at all.

I did not feel justified in resisting such an appeal to my trustfulness, and agreed to his proposal.

A few days ago I received the following letter, which I shall be glad if you will kindly publish, along with my answer.

Sir F. A. Abel is the inventor of the dynamite water-cartridge, and not myself, as might be inferred from the article in *Glückauf*.

W. GALLOWAY

Freiburg, October 2, 1885

HOCHGEEHRTER FREUND.—Es war mir unmöglich wieder nach Cardiff zurückzukehren da wir uns zu lange im Durhamreviere aufgehalten hatten und die Zeit meiner zulässigen Bleibens in England sich allzusehr dem Ende zuneigte. Leider bin ich dadurch um das Vergnügen gekommen noch einmal mit Ihnen persönlich verkehren zu können, doch hoffe ich, dass wir uns bald einmal wieder sehen, vielleicht in Zwickau.

Die Schiessversuche mit Wasserbesatz und Pulver———der Versuchsstrecke haben sowohl in Zwickau als auch in Neunkirchen zu guten Resultaten in so fern geführt als die Gasen nicht entzündet wurden. Versuche mit Pulver und Wasserbesatz in der Plauitzer Kohle ergeben aber in so fern keine guten Resultate, als die Schüsse nicht werfen.

Leider hat Assesser Nonne, welcher den Versuchen beiwohnte, ganz gegen unsere Verabredung sogleich die Resultate dieser ersten Versuche in einer kurzen Notiz im *Glückauf* veröffent licht, jedoch ohne ihre Priorität zu nah zu treten, da Sie besonders darin erwähnt sind. Ich hatte ausdrücklich vor jeder Publication gewarnt ehe die Ihrige nicht erschienen sei, ein ordinärer Character kümmert sich aber um so etwas nicht.

Bei späterer Veröffentlichung der Zwickauer Versuche kann eventuel darauf Bezug genommen werden.

Nochmals für alle Liebe und Freundschaft, die sie mir so vielfältig erwiesen haben bestens dankend,

Verbleibe ich mit herzlichem Glückauf,

Ihr,

Ergebenster,

C. G. KREISCHER

Herrn Bergingenieur Galloway, Cardiff

Cardiff, October 9, 1885

DEAR PROFESSOR KREISCHER,—I have received your letter of the 2nd inst. I observe that the friends to whom you sent a description of the shot-firing experiments have violated the conditions under which I gave you permission to make your communication to them by already publishing their results, as if

they were in some sort original. You mention as a kind of palliative that, although my priority is not distinctly admitted, my name is mentioned in a prominent manner.

Personally I consider this a very small affair. Long experience of having my name mentioned in a similar manner, or mixed up with the names of others, or altogether omitted in connection with certain coal-dust matters in which I have undeniable priority, has hardened me; and I confess that this part of your letter gave me no concern. But although I could afford to pass it over in this way as far as I am myself concerned, I cannot adopt the same course when the interests of some of the members of the Royal Commission on Accidents in Mines are also at stake.

I must therefore ask you to give me a token of your good faith by restraining your friends from publishing anything further until the English Royal Commissioners shall have seen fit to make known the results obtained here. At the same time also I would suggest it as a simple matter of duty on your part to take immediate steps to let it be known to those before whom your friends' communications have appeared that the credit, if any, of the original investigations in this case rests with Sir Frederick Abel and Mr. W. Thomas Lewis quite as much as with me. Believe me yours very faithfully,

W. GALLOWAY

Herr Bergrath Kreischer, Professor der Bergbaukunde, Freiberg, Sachsen

The Resting Position of Oysters

IN books on Conchology, such as Woodward's "Manual of the Mollusca" and Jeffrey's "British Conchology," it is stated that the oyster rests in the natural state on its left valve, which is the larger and more convex. In this respect it is pointed out the oyster differs from the animals belonging to the genera Pecten and Anomia, which rest on the right valve, the Anomias being firmly attached by muscle with the flat right valve applied closely to the surface of attachment. In his lecture on oysters at the Royal Institution, which was published in Nos. 1 and 2 of the *English Illustrated Magazine*, Prof. Huxley also states that oysters rest on the left or convex valve, the flat right valve acting as a kind of operculum. Examination of oysters from the Firth of Forth has convinced me that this statement is erroneous. I do not know on what evidence the current belief of conchologists is founded. The evidence which appears to me conclusive is that the right flat valve is always quite clean, while the convex valve is covered with worm tubes, *Styela grossularia*, and Hydroids. The latter are in this connection the most important; it would be impossible for specimens of Sertularia and Thuiaria 4 or 5 inches long to grow, as I have found them on almost every oyster, in the central part of the left valve, if that valve were the lower in position. On examining Pectens I found that they resembled the oyster in the contrast between the surfaces of the two valves, the upper convex one being covered with Balanus and other fixed animals, the lower being almost clean. It is generally stated that the Pecten lies on its right valve; if this statement rests on the evidence afforded by the condition of the surface of the valves the same criterion applied to the oyster leads to the same conclusion, that the right valve is the lower. I have never seen a young oyster in the attached condition: Huxley states that it is the left valve which is fixed; in papers on the embryology of the oyster I have not yet been able to find any definite information on the point. Whether it is the right or left valve that becomes attached when the larva assumes the sessile condition I cannot therefore say of my own knowledge, but with regard to the adult oyster it seems to me certain that the current belief is caused by the repetition of an error. My attention was first called to this point by my assistant, Mr. John Walker, who tells me that the opinion of the fishermen at Newhaven is divided on the point, some saying that the convex valve, others that the flat valve, is the lower.

J. T. CUNNINGHAM

Scottish Marine Station, Granton, October 14

Two Generalisations

TWO generalisations seem to have been staring us in the face for some time, and yet I have seen no one give them a look of recognition; they may be phantasms, but they seem solid enough:—

(1) That the number of elements is infinite; the most readily-formed types of ethereal vortices being the commonest, but our knowledge of them being only limited by the scarceness of the more complex forms, and not by any limit to the infinite varieties of complexity that may exist. Their relative commonness being analogous to the relative sizes of the bodies of the solar system; a few large, and always recognisable, and a greater number of examples as we descend in size to mere meteors. We already see that there are far more rare elements known than common ones.

(2) That the reduction of an electric current to heat in an imperfect conductor is solely due to the independent heat-motions of the molecules, which check and divert more and more of the current as their motions are larger; if there were no pre-existing heat-motions there would be nothing to resist a complete transmission of the current motion, and hence there would be no limit to conduction at the zero of temperature except the cohesion of the material.

Bromley, Kent W. M. FLINDERS PETRIE

Meteors

ON the morning of October 13, at 2h. 26m., I saw a fine meteor giving a bright flash at the end point and leaving a streak for about 12 seconds. It shot from the Lynx towards the pointers in Ursa Major, and while carefully fixing its direction relatively to the stars near, another conspicuous meteor, about as bright as Jupiter, crossed the lingering streak in a path but slightly inclined to it and of nearly similar length. I have never before observed two large meteors almost simultaneous and with paths so nearly identical.

I subjoin the observed paths of these meteors, also of five other bolides recently noted here during the progress of my habitual watches for shooting stars:—

1885	G.M.T. h. m.	Mag.	Path From	Path To	Length	Radiant
			° °	° °	°	° °
Sept. 9	15 48	♃	149 +82	152 +64	18	335+71
,, 15	15 11	♃	37 + 6½	26½+ 7	10½	70+ 4
Oct. 7	10 51	♃	51½+22	71½+24	18	31+18
,, 8	15 9	♃	155 +53	162½+46½	8	42+55
,, 12	14 26	♀	119 +51	151 +60½	20	88+18
,, 12	14 26	♃	119½+50	143 +60½	16½	103+33
,, 16	16 35	♃	213 +47½	226 +41	11	143+49

The radiant points are derived in each case by combination with many other meteors registered on about the same nights. I have seen 357 meteors since early in September, and those selected in the above table comprise all the brighter specimens estimated to equal Jupiter. W. F. DENNING

Bristol, October 17

Statigrams

THE increasing use of graphic representations of statistics by means of lines, areas, &c., seems to render it convenient to have some word which would specially designate diagrams exhibiting the progress and tendencies of the numerous tables of figures which do not pretend to strict scientific accuracy. The word *diagram* is used in most elastic senses and by all sorts and conditions of men.

May I suggest the word *statigraph* as a definite and convenient one for adoption? This might be sometimes shortened to *graph;* whereas *statigram*, if preferred, would not admit of this abbreviation. Most, if not all, graphic results of statists, economists, anthropologists, &c., might thus be termed *graphs*, whilst mathematicians and the experimental men of science would be left with the use of their own words, such as *curves*, *indicator diagrams*, &c. Each class would possess its own degree and limits of accuracy: mathematical precision and the doctrine of energy would apply to the latter, but *graphs* would be understood to involve human elements with intricate factors whose recognition or relationships the statistics are intended to elucidate and compare rather than to define and measure.

12, Merton St., Oxford J. F. HEYES

THE GEOLOGICAL SURVEY OF BELGIUM

PROBABLY no country of Europe has had its geology more attentively studied and mapped than Belgium. From the early labours of the veteran and pioneer